XVII. *Reflections on the Communication of Motion by Impact and Gravity.* *By the Rev.* Iſaac Milner, *M. A.* *Fellow of* Queen's College, Cambridge. *Communicated by* Anthony Shepherd, *D. D. F. R. S. and Plumian Profeſſor at* Cambridge.

Read Feb. 26, 1778.

THE theory of moving bodies was little underſtood by the philoſophers who lived in the ſixteenth century. They obſerved, that a body, once put into motion, continued to move for ſome time after the force was impreſſed; but they argued very ſtrangely from this ordinary phænomenon. Far from conſidering the air as a reſiſting medium; they ſuppoſed with ARISTOTLE and the ancients, that it was the perpetual influx of the parts of the atmoſphere which continued to urge the body forward and preſerve its motion. When a body is projected in any direction inclined to the horizon, the gravity of its parts is always obſerved to bend the direction of its motion into a curve line; and becauſe this gravity remains invariably the ſame, whatever the force of projection be, in very ſwift motions, the figure deſcribed may approach very nearly to a right line.

This laſt circumſtance induced ſome of the philoſophers we are ſpeaking of to believe, that a cannon ball, for inſtance, always moves in the ſame ſtrait line till its velocity is entirely deſtroyed; and that afterwards it deſcends towards the earth in a direction perpendicular to the horizon. Others thought they mended the matter by ſuſpending the action of gravity for a certain period only; by allowing the latter part of the path to be curvilinear; and laſtly, the body to deſcend to the earth in a ſtraight line, as in the former caſe. We, in theſe days, who have ſeen the gradual improvements in mechanics from time to time, are not ſurprized, that men, in the infancy of that ſcience, ſhould have embraced abſurd and ridiculous principles: we rather wonder, how the author (a) of the notion juſt mentioned was able to form any juſt eſtimate of the horizontal ranges of projectiles, and to diſcover their maxima. Whether by conjecture, or probability of induction, we are unable to determine; but ſo it was, TARTALEA affirmed, what has ſince been found true upon unexceptionable evidence, that the amplitudes of projectiles upon the horizon are always greateſt when the angles of projection are equal to 45°. But the praiſe of this diſcovery, as well as whatever elſe relates to the accelerated motions of bodies near the ſurface of the earth, is juſtly due to the incomparable GALILEO. The theory of mechanics had received no conſiderable improvement ſince the time of ARCHIMEDES, when this ſurprizing genius appeared in the former part of the ſeventeenth century. He diſcarded the peripatetic philoſophy; explained the whole doctrine of accelerated motion and of projectiles: in a word, he ſo much exhauſted the ſubject, that the beſt treatiſes we have at this day are little more than a repetition of GALILEO's diſcoveries.

(a) NICH. TARTALEA.

This philoſopher, as far as we know, never attempted to inveſtigate the laws by which motion is communicated from one body to another. The celebrated DES CARTES is the firſt we hear of who gave any attention to the ſubject; and the reſult of his enquiries is what might reaſonably be expected from ſo whimſical and romantic a genius; he blundered in this, as in all other caſes, where he was not confined to pure mathematical reaſonings. Our countryman, Dr. WALLIS, made a real progreſs in this ſcience, by diſcovering that fundamental law in the communication of motion, *viz.* that action is equal to re-action, and always in contrary directions: WREN, HUYGENS, confirmed the ſame thing; and the whole theory of the colliſion of bodies, and their mutual actions upon one another, ſeemed to be advancing faſt towards perfection.

But a new opinion was now ſtarted by M. LEIBNITZ concerning the forces of bodies in motion. The force of a body in motion and its momentum had hitherto been conſidered as ſynonymous terms, and had alike been meaſured by the quantity of matter and velocity conjointly. On the contrary, LEIBNITZ and his followers affirmed, that the force was proportional to the quantity of matter in the moving body and the ſquare of its velocity. It is needleſs to relate all that paſſed on both ſides: ſo material an oppoſition in ſentiment neceſſarily produced very warm contention; and, as it generally happens in other diſputes, we do not hear of any conviction being produced on either ſide.

After ſurveying the arguments of the diſputants, it is not eaſy to ſay, whether the agitation of the queſtion before us has contributed to retard or advance the progreſs of truth and ſcience. On the one hand, many ingenious experiments have been made, many curious problems invented and reſolved, which probably would never once have been thought of by men who were in the purſuit of truth in a more cool and deliberate way: and, on the other hand, it may juſtly be affirmed, that the violence of prejudice and party-ſpirit has ſo much clouded the reaſonings of the beſt writers, that we ſenſibly feel their influence to this day. I need not diſſemble:

it is a ſerious perſuaſion, that the laws by which motion is communicated are ſtill very materially miſtaken by ſenſible perſons, that induced me to throw together the following hints, and to lay them before the Royal Society. The right underſtanding of theſe laws is of the laſt importance in practice: the good or bad ſucceſs of ſome very expenſive projects has depended upon it; and certain excellent artiſts have been diſappointed in the execution of their plans, and unable to reconcile the apparent contradiction between theory and experiment. From the length of time, which has elapſed ſince LEIBNITZ firſt advanced his new opinions, and the abilities of the philoſophers who engaged in the conteſt, one might have expected, that the whole matter would long before this have been cleared up in a ſatisfactory manner; eſpecially when we conſider, that the communication of motion from one body to another is what every moment happens before our eyes, and that particular experiments are made in this doctrine with the greateſt ſimplicity and convenience. This part of rational mechanics however is not yet generally underſtood, as we may fairly preſume from the difference of opinion which ſtill ſubſiſts among the learned. I freely own, it appears to me, that no new experiments are wanting; no new geometrical reaſonings or conſtructions: the improved parts of geometry have been already applied to the theory of motion in numberleſs caſes, and a variety of well atteſted experiments have been clearly explained to us by authors. The laws of motion, in certain caſes, are inconteſtable, and no author of eminence contradicts them: it is from a miſtaken application of theſe laws that a difference of opinion has ariſen. It is obvious, that the laws of motion, as deſcribed by Sir ISAAC NEWTON, may, in a certain ſenſe, be founded on experiment; and yet, if they are extended to caſes where they cannot be applied, the concluſions muſt ſtill be erroneous. My deſign in theſe pages is to point out diſtinctly what is real in this difference of opinion from what is merely verbal, and to explain the cauſes of it. This, which perhaps will appear to have never been done with ſufficient preciſion, ſeems to be the moſt effectual way of preventing miſtakes. Geometry and algebra will lead us wrong, if our principles are ill founded: experiment itſelf, if we are not extremely careful, will deceive us in forming a general deduction, or what is called a law of nature. The controverſial writings of the moſt able authors will embarraſs and perplex our judgements; but when we have once diſcovered the grounds of their mutual miſtakes and miſapprehenſions, there is reaſon to think, that we

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ſhall both underſtand the ſubject better than we did before, and be more on our guard for the future.

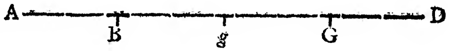
The firſt law of motion, as expreſſed by Sir ISAAC NEWTON, is unexceptionable: nobody denies that a body perſeveres in a ſtate of reſt or uniform motion in a right line, till affected by ſome external influence. It is the third law of motion which has produced all this confuſion and perplexity. "Actioni contrariam ſemper et "æqualem eſſe reactionem: ſive corporum duorum ac"tiones in ſe mutuo ſemper æquales et in partes con"trarias dirigi." Theſe words of Sir ISAAC NEWTON convey to us as clear an idea as can poſſibly be conceived with ſo much conciſeneſs. It muſt however be confeſſed, that his illuſtration is not ſo very perſpicuous(b). To ſay, that when a man preſſes a ſtone with his finger, his finger is equally preſſed; and when a horſe draws a ſtone by a cord, the horſe is drawn equally backwards towards the ſtone; is a moſt indiſtinct and popular way of ſpeaking, and can never make evident what was before not underſtood.

(b) Quicquid premit vel trahit alterum, tantundem ab eo premitur vel trahitur. Si quis lapidem digito premit, premitur et hujus digitus a lapide. Si equus lapidem funi alligatum trahit, retrahitur etiam et equus (ut ita dicam) æqualiter in lapidem: nam funis utrinque diſtentus eodem relaxandi ſe conatu urgebit, equum verſus lapidem, ac lapidem verſus equum; tantumque impediet progreſſum unius quantum promovet progreſſum alterius, &c. NEWTON Princip.

Some uſeful writers, who have copied after Sir ISAAC NEWTON, have talked in the ſame way; and only increaſed the ambiguity by being more diffuſe. Mr. MACLAURIN himſelf, who engaged very warmly in this debate with the foreign mathematicians, and who, to ſay the truth, ſeems to have underſtood the nature of the controverſy better than any one elſe, is frequently unguarded in his expreſſion. In chap. II. book 2. of his account of NEWTON's diſcoveries, he is deſcribing the laws of motion for the firſt time, and one naturally expects a more than ordinary preciſion and exactneſs. There he blames, very juſtly, the oppoſers of the Newtonian definition of motion for miſtaking the direction in which the motion, loſt or communicated, ought always to be eſtimated. But in p. 122 (c), he thus expreſſes himſelf: "When two "bodies meet, each endeavours to perſevere in its ſtate, "and reſiſts any change; and becauſe the change, which "is produced in either, may be equally meaſured by the "action, which it exerts upon the other, or by the re"ſiſtance, which it meets with from it, *it follows*, that "the changes produced in the motions of each are "equal; but are made in contrary directions." I cannot poſſibly conceive, that ſo ſkilful and accurate a philoſopher could believe, that the third law of motion was an inference of reaſon, excluſive of all experiment; and yet, if words have any meaning at all, the above quotation inclines us to think ſo. It is true, the change which is produced in either body may be meaſured by the action which it exerts upon the other, or by the reſiſtance which it meets with from the ſame: but what are we to underſtand by action or reſiſtance, until they are explained by more intelligible terms? or, when they are explained by terms which do not neceſſarily imply the ſame thing, how do we know that their meaſures are equal, or that they are made in contrary directions, until theſe truths be eſtabliſhed by experiments? A law of nature is not merely a deduction of reaſon: it muſt be proved, either at once and directly, by ſome ſimple and deciſive experiments; or if that cannot be done, by ſuch experiments as enable us to collect its exiſtence by the aſſiſtance of geometry. However obvious theſe reflections may appear, I thought it neceſſary to take notice of MACLAURIN's aſſertion; becauſe in conſequence of that and ſimilar expreſſions, young philoſophers are extremely puzzled in the beginning of their ſtudies, and becauſe I have known ſome, who are more experienced, affirm, that the third law of motion is nothing more than a definition. I now proceed to the conſideration of particular caſes.

(c) Octavo edition.

CASE THE FIRST. Suppoſe A and B to repreſent the magnitudes of two ſpherical bodies, and *a* and *b* their reſpective velocities in the ſame direction; ſuppoſe *a* to be greater than *b*, and A will overtake B; and if the bodies are non-elaſtic, they will proceed together in the ſame direction as one maſs: if they are perfectly elaſtic, whatever effect has already been produced by the colliſion, will be repeated; and, becauſe in the firſt caſe there is no relative velocity after the ſtroke, in the ſecond the relative velocity before and after the ſtroke will be the ſame, and in contrary directions; and in either caſe, the motion loſt by the ſtriking body is found to be always equal to the motion communicated to B, and in a contrary direction. In this ſenſe action is equal to re-action; and every experiment which has yet been produced, where a clear judgment could be formed of the effect, has confirmed the ſame thing. All the experiments which are uſually brought to determine the impreſſions made upon ſoft bodies, as ſnow, clay, &c. are abſolutely unfit for the purpoſe. The circumſtances, which take place in the production of theſe effects, are ſuch as we can never diſcover. The directions in which the particles recede, the velocities they acquire, their mutual actions upon one another, and laſtly, the time, in which theſe effects are performed, are all beyond the reach of computation. The other principle, that the relative velocity of A and B is not altered by the ſtroke, is neither to be demonſtrated nor confirmed by experience; it is a direct conſequence of the definition of elaſticity. Again, ſuppoſe α and β to repreſent the reſpective velocities of A and B after the ſtroke, and from theſe *data* it is eaſily inferred, that $A\alpha^2 + B\beta^2 = Aa^2 + Bb^2$: for $a-b$ is equal to $\beta-\alpha$, becauſe $a-b$ is the relative velocity before, and $\beta-\alpha$ the relative velocity after, the ſtroke. And $Aa+Bb$ is equal to $A\alpha+B\beta$, becauſe theſe quantities repreſent the ſum of the motions before and after the ſtroke reſpectively; and from theſe equations the above equation is deduced, ſhewing, that in elaſtic bodies the ſum of the two bodies multiplied by the ſquares of their abſolute velocities, is not altered by the ſtroke.



The ſame theorem may be demonſtrated geometrically in the following manner. Let the velocities of A and B be repreſented by AD, AB, reſpectively; and let G be their center of gravity, when placed at B and D; the velocity of A after the ſtroke will be repreſented by B*g*, if G*g* be taken equal to GD, and the velocity of B by AB+2BG. From the nature of the center of gravity $A \times GD = B \times BG$, and $A \times GD \times 4AG = B \times BG \times 4AG =$

$\mathrm{B} \times \overline{4\mathrm{BG}^2 + 4\mathrm{BG} \times \mathrm{AB}}$. Add to both ſides $\mathrm{A} \times \mathrm{A}g^2 + \mathrm{B} \times \mathrm{AB}^2$, and we ſhall have $\mathrm{A} \times \mathrm{AD}^2 + \mathrm{B} \times \mathrm{AB}^2 = \mathrm{A} \times \mathrm{A}g^2 + \mathrm{B} \times \overline{\mathrm{AB} + 2\mathrm{BG}}^2$.

We are not to wonder, therefore, upon making trials with perfectly elaſtic bodies, if any ſuch exiſted, were we always to find their *vires vivæ*, as the foreigners expreſs themſelves, neither increaſed nor diminiſhed by the ſtroke. They define the force of bodies in motion, or their *vis viva*, to be in a compound ratio of their quantities of matter, and the ſquares of their velocities; and certainly ſuch a definition implies no contradiction or impoſſibility. The term force, in a looſe and ordinary way of ſpeaking, conveys to us no determinate idea at all, and therefore, until it be defined, is incapable of being uſed to any good purpoſe in philoſophy: whether this or that definition come nearer to the general ſenſe in which it is uſed indiſtinctly enough in common language, is entirely another queſtion. We may go farther, and add, that in their uſe of the words, becauſe the ſum of the forces of elaſtic bodies is never affected by the ſtroke, it is not unnatural to ſay, that action is therefore equal to re-action, and that no force is loſt by one body but what is communicated to the other. But if we will go ſo far, and thereby change the meaning of the terms action and re-action and their meaſures, we ought at leaſt to guard our readers from miſtaking us, however convenient ſuch modes of expreſſion may appear. Becauſe $A\alpha^2+B\beta^2$ is equal to Aa^2+Bb^2, it is true that no force is loſt by A but what is communicated to B; but not in the ſame ſenſe in which it was affirmed that no motion is loſt by A but what is communicated to B. In that caſe the ſquares of their abſolute velocities are underſtood; in this, their velocities reduced to the ſame direction. However, no material ill conſequence can poſſibly ariſe from ſuch a notion of action and re-action, as long as the queſtion is ſuppoſed to concern only elaſtic bodies: but real miſchief is done, and the debate ceaſes to be verbal, whenever the law of the equality of action and re-action is ſaid to take place in the colliſions of all ſorts of bodies.

CASE THE SECOND. But the truth of theſe remarks, and the neceſſity of attending to the preciſe uſe of terms, will appear in a ſtill ſtronger light, if we conſider the ſolution of a problem given us by J. BERNOULLI(d).

Suppoſe that two equal and ſpherical bodies, A and B, ſtruck at once in the direction CD perpendicular to the line joining the centers of A and B with a velocity repreſented by a. Let the quantity of matter in C be called m, and the quantity of matter in A or B, n: let the velo-

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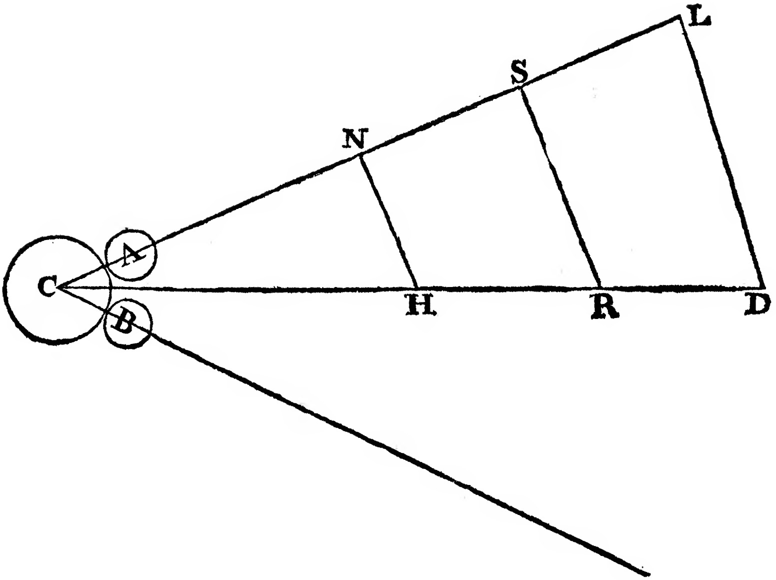
(d) Diſcours ſur le mouvement.

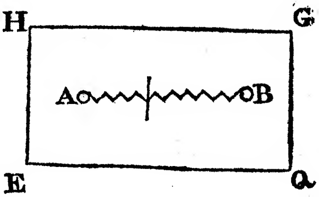


city of C after the ſtroke be repreſented by x, and that of A or B in the direction AC or CB by y, and ſuppoſe $p : q$ :: rad. : coſin. LCD. Then, becauſe ma the quantity of motion before the ſtroke is equal to $mx+\frac{2qny}{p}$, the quantity of motion after the ſtroke, and ma^2 is equal to mx^2+2ny^2, becauſe the quantity of force is not altered by the colliſion; he eaſily finds $x=\frac{p^2ma-2q^2na}{p^2m+2q^2n}$ and $y=\frac{2pqma}{p^2m+2q^2n}$.

There is no problem which deſerves to be more conſidered than this by a perſon deſirous of having a clear idea of the grounds of that contention which has ſubſiſted ſo many years. We here ſee BERNOULLI taking it for granted, that the quantity of force in elaſtic bodies is no ways affected by their mutual actions, whether direct or oblique; and the moſt ſurprizing circumſtance is, that he ſhould not ſo much as hint at any apparent difficulty in the preſent caſe, after he had been ſo very diffuſe in illuſtrating others which were much more ſimple. No doubt he believed this principle to be a direct conſequence of the equality of action and re-action, and therefore it is plain he could not mean the ſame things by thoſe terms as we do at preſent. He believes no force is gained or loſt by impact; he defines force by quantity of matter and ſquare of the velocity conjointly; and in eſtimating the velocity, he pays no regard to the direction in which the bodies are moved. Let us not cavil at his words: we cannot miſtake his meaning. The queſtion is, how far theſe notions are agreeable to experience; how far they are conſiſtent with ſome other principles which are inconteſtable, and which he himſelf has admitted: for inſtance, he admits it as an undoubted principle, that the quantity of motion in any ſyſtem of bodies is preſerved invariable, when eſtimated in a given direction, in all their colliſions and mutual actions upon one another; and in this he entirely agrees with the followers of Sir

ISAAC NEWTON. Let us attend to the conſequences of theſe two different principles in the very caſe propoſed by J. BERNOULLI. And firſt, becauſe $ma=mx+\frac{2qny}{p}$, by tranſpoſition we have $m\times\overline{a-x}=\frac{2qny}{p}$, which is ſaying no more than that the motion loſt by C is equal to the ſum of the motions gained by A and B, eſtimated in the ſame direction CD. By a ſimilar proceſs from the ſecond equation, we deduce $m\times\overline{a+x}\times\overline{a-x}=2nq^2$; and therefore the compariſon of the two equations gives $\frac{q\times\overline{a+x}}{p}=y$. The quantity y therefore, or the velocity of A or B after the ſtroke, muſt neceſſarily be equal to the ſum of the two quantities $\frac{qa}{p}$ and $\frac{qx}{p}$. In the figure, let CD repreſent the velocity of C before the ſtroke, and CH the velocity after it, and let fall the perpendiculars Hn, DL, upon the direction AC. It eaſily appears, that Cn is equal to $\frac{qx}{p}$ and CL equal to $\frac{qa}{p}$, becauſe CH : CN :: CD : CL :: rad. : coſ. LCD :: $q:p$. And now the whole controverſy is reduced into a narrow compaſs; for whether the two principles aſſumed by this author be conſiſtent with experience or not; it is impoſſible they ſhould be conſiſtent with one another, unleſs Cn+CL ſhall be found to meaſure the velocity of A in the direction CL. Suppoſe Cr to be the velocity of C after impact, when all the bodies are perfectly hard, and letting fall the perpendicular rs; $\mathrm{C}s$ will be the velocity acquired by A in that caſe; and, univerſally, the velocity acquired by A will be equal to $\mathrm{C}s+\frac{\mathrm{C}s}{m}$, if the elaſticity of the bodies be to perfect elaſticity as $1:m$. In order to determine, therefore, when $\mathrm{C}n+\mathrm{CL}$ can poſſibly be equal to $\mathrm{C}s+\frac{\mathrm{C}s}{m}$, or, which is the ſame thing, $\mathrm{L}s+\mathrm{C}n$ equal to $\frac{\mathrm{C}s}{m}$, we are to conſider that $ns:\mathrm{L}s::1:m$: and becauſe $\mathrm{C}n$ is equal to $\mathrm{C}s-sn$, $\mathrm{C}n=\mathrm{C}s-\frac{\mathrm{L}s}{m}$, and it is obvious that $\mathrm{C}s+\mathrm{L}s-\frac{\mathrm{L}s}{m}$ can never be equal to $\frac{\mathrm{C}s}{m}$, unleſs m be taken equal to unity, and BERNOULLI's hypotheſis is plainly impoſſible in all caſes where the bodies are not ſuppoſed perfectly elaſtic.

But though we confeſs the learned author, who firſt ſolved the problem we have been conſidering, deſerves no commendation for propoſing in a general form what ought to have been reſtrained to a particular caſe, yet it will by no means follow, that every argument which has been advanced againſt this doctrine is either intelligible or ſatisfactory. Of all the objections and experiments which have been ſtarted and contrived to refute the new opinions of the German philoſophers, there is none which carries a greater degree of plauſibility along with it, than a celebrated invention of Mr. MACLAURIN. It is extremely ſimple, eaſy to be deſcribed; and I do not find that it has ever been anſwered by any of the advocates for the new doctrine of forces.



" Let A and B be two equal " bodies that are ſeparated from " each other by ſprings inter- " poſed between them, in a " ſpace EFGH, which in the " mean time proceeds uniformly in the direction BA (in " which line the ſprings act) with a velocity as 1; and " ſuppoſe that the ſprings impreſs on the equal bodies A " and B equal velocities, in oppoſite directions, that are " each as 1. Then the abſolute velocity of A (which " was as 1) will now be as 2; and according to the new " doctrine its force as 4: whereas the abſolute velocity " and force of B (which was as 1) will now be deſtroyed; " ſo that the action of the ſprings adds to A a force as 3, " and ſubducts from the equal body B a force as one " only; and yet it ſeems manifeſt, that the actions of the " ſprings on theſe equal bodies ought to be equal, and " M. BERNOULLI expreſsly owns them to be ſo(e). I ſhall only juſt obſerve, that if M. BERNOULLI expreſsly owns, that ſprings, interpoſed between two bodies in a ſpace, which is carried uniformly in the direction in which the ſprings act, will always generate equal forces in the bodies according to his own definition of that term, he talks more inconſiſtently than I have obſerved him to do: on the contrary, if I could find that he has anſwered this famous argument (which Dr. JURIN propoſed over again in Phil. Tranſ. vol. XLIII. with a conditional promiſe of embracing the Leibnitzian doctrine) by ſimply ſaying, that ſprings he conſiders as motive forces, or, when the bodies are equal, as accelerating forces; and that their actions are equal, when in equal times they generate equal velocities, but not neceſſarily equal forces, in the equal bodies; I ſhould not make the leaſt ſcruple to own that I thought his reaſoning ſolid and concluſive, and his diſtinctions a full anſwer to every objection of that ſort(f).

(e) Book II. chap. 2. Account of NEWTON's diſcoveries.

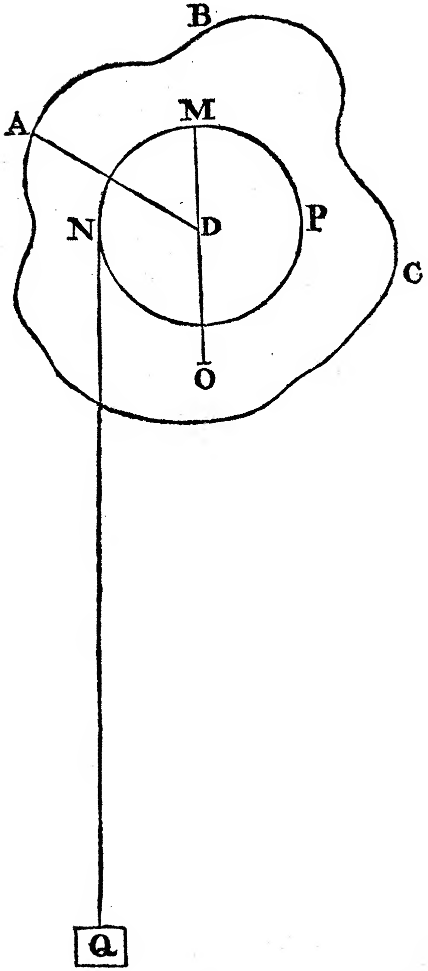
CASE THE THIRD. The two preceding caſes are curious examples of the force of prejudice and party-ſpirit. In the latter particularly it does not appear that J. BERNOULLI

(f) No doubt MACLAURIN refers to the following paſſage of BERNOULLI, "La force du choc, ou de l'action des corps les uns ſur les autres, depend uniquement de leurs viteſſes reſpectives; or il eſt viſible que les viteſſes reſpectives des corps ne changent pas avant le choc, ſoit que le plan ou l'eſpace qui les contient ſoit ſans mouvement, ſoit qu'il ſe meuve uniformement, ſuivant une direction donnée, les viteſſes reſpectives ſeront donc encore les mêmes après le choc."

This quotation puts the matter beyond diſpute. It is plain, BERNOULLI, though he does make uſe of the word action, is only ſpeaking of the motion loſt or communicated, and the relative velocities of the bodies: there is not the moſt diſtant hint at the change in their abſolute forces.

NOULLI knew the preſervation of the *vires vivæ* to be an infallible conſequence of perfect elaſticity in bodies; or indeed that he had any other reaſon for taking that principle for granted, but becauſe he was not able to prove it. All the inſtances that are uſually brought on both ſides are to be treated in a ſimilar way. The meaning of the terms muſt firſt be defined; then the principles aſſumed explained; and if we cannot tell at firſt ſight, whether they are agreeable to experience or not, as is frequently the caſe; we muſt examine into their conſequences by the aſſiſtance of geometry, and we ſhall at laſt arrive at ſome ſimple principle, the exiſtence of which is neceſſarily implied in the original hypotheſis. The colliſion of ſpherical bodies is the moſt ſimple way of communicating motion from one to another; and therefore ſuch examples are better adapted to throw light on a diſputable queſtion, than where the ſuppoſitions are more perplexed with mechanical contrivances. Beſides, when the theory of mechanics is well underſtood, and the foundations of error diſcovered, the ſame reaſonings are eaſily transferred to other caſes, and ſimilar precautions applied. Indeed practical artiſts have little to do with the ſudden communication of motion by impact. The colliſions of bodies are too violent operations to enter into the compoſition of uſeful machines, in which motions are rather to be preſerved by the gradual effects of weights and preſſures. An accurate knowledge therefore of theſe effects is more eſſential to the intereſts of ſociety; and the only way of arriving at ſuch a knowledge is always to diſtinguiſh thoſe principles which nobody denies, from thoſe others which are found to take place only in ſome particular circumſtances. The following problem was propoſed, and a ſolution given to it long ago, by D. BERNOULLI (g).

(g) Comment. Petrop. tom. II.



“ Sit grave aliquod
“ cujuſcunque figuræ
“ CBA, cujus centrum
“ gravitatis ſit D; ex quo
“ et radio D M deſcriptus
“ intelligatur circulus
“ MNP, cui filum cir-
“ cumvolutum eſt PMN,
“ cujus fili extremitati
“ appenſum ſit pondus
“ Q, quod deſcenſu ſuo
“ grave CBA in gyrum
“ agit circum centrum
“ gravitatis D, dico velo-
“ citatem corporis Q ſe-
“ quentem in modum
“ determinari poſſe. Sit
“ M D $= a$; conſideretur
“ corpus ſuſpenſum ex
“ puncto M oſcillari, eſſe-
“ que centrum oſcilla-
“ tionis in O, ſitque
“ DO $= b$, pondus gravis totius CBA $=$ P, pondus corporis
“ appenſi $= p$; altitudo ex quâ corpus Q delapſum eſt $=$ R;
“ altitudo quæſita per quam grave aliquod cadendo ac-

“ quirere poſſit velocitatem corporis $\text{Q}=z$; dico fore $z=$
“ $\frac{ap\text{R}}{ap+b\text{P}}$, et ſi tempus quo corpus naturaliter cadit, per
“ altitudinem R dicatur t, erit tempus inſumptum a cor-
“ pore $\text{Q}=t\sqrt{\frac{ap+b\text{P}}{ap}}$, id quod experientiæ conforme eſſe
“ plurimis inſtitutis experimentis ſemper inveni.”

Both theſe concluſions are derived by this author from the principle, which they call the *conſervatio virium vivarum*; but as he has not given us the ſeveral ſteps of his reaſoning, it may be uſeful to ſupply them here, before we proceed to make any remarks upon his ſolution. And firſt, ſuppoſe the axis at D to be perpendicular to the plane of the figure, and conceive the whole body to be reſolved into an indefinite number of priſmatic particles, each of which is perpendicular to the ſame plane. Let E repreſent the ſum of all the particles multiplied by the ſquares of their reſpective diſtances from the axis; and E ſhall be equal to $\text{P}\times ab$, as is demonſtrated by all the writers who treat of the center of gyration. Let v be the velocity which is actually acquired by Q after it has deſcended through the ſpace R; V the velocity which it would have acquired by the ſame deſcent, provided the body had fallen freely by its gravity; and becauſe the *vires vivæ* are incapable of diminution or increaſe, we have $pV^2 = pv^2 + \frac{Pabv^2}{a^2}$. For ſince v is the velocity of Q at a certain period of its deſcent, and is to the velocity of any priſmatic particle in the body as the diſtance mD from the axis to the diſtance of that particle from the ſame, it is evident that $\frac{Pabv^2}{a^2}$ will truly repreſent the ſum of all the particles multiplied by the ſquares of their velocities. V^2 is therefore to v^2 as $ap + bP$ to ap, and the whole force of gravity is to the force which accelerates the motion of Q in the ſame ratio, becauſe in uniformly accelerated motions, when the ſpaces deſcribed are the ſame, the accelerating forces are in the duplicate ratio of the velocities. It is obvious, that the motion of Q is uniformly accelerated, becauſe the velocity acquired by any deſcent is to the velocity of any point in the body always in the ſame ratio; and therefore the action of Q upon the body is the ſame as if both were at reſt. Farther, the altitude z through which a heavy body muſt fall to acquire the velocity v is plainly equal to $R \times \frac{ap}{ap + bP}$, for the altitudes Z and R are inverſely, as the forces which generate the equal velocities. Laſtly, the time of Q's deſcent is equal to $t \times \sqrt{\frac{ap + bP}{ap}}$; becauſe the times are always in the ſub-duplicate ratio of the ſpaces directly, and forces inverſely.

It is now extremely eaſy to trace theſe expreſſions back again in a contrary order, and to ſhew, that if theſe laſt equations are true, the original one muſt be true alſo; that $p \times V^2$ muſt neceſſarily be equal to $\frac{Pabv^2}{a^2} + pv^2$, or, which is the ſame thing, that the body Q multiplied into the ſquare of its velocity, and added to the ſum of all the products which ariſe by multiplying every particle into the ſquare of its reſpective velocity, is equal to the body Q multiplied by the ſquare of the velocity which it would have acquired by the ſame deſcent *in vacuo*.

Now this is to give the argument its full force; and ſince the concluſions are confirmed by repeated experiments, as the author himſelf aſſures us, it is preſumed, that the premiſes can be liable to no juſt exception. If we do not think with the advocates for this doctrine, that the *vires vivæ* muſt always remain the ſame from the thing itſelf, they will force our aſſent by the teſtimony of experience, and oblige us to admit their principles when we find it impoſſible to deny the conſequences.

A prudent philoſopher is always afraid to pronounce generally concerning the exiſtence of cauſes, which are attended with a variety of circumſtances, and are complex in their operations. To ſay that the quantity of force in bodies remains invariably the ſame, ſeems to be a propoſition of this kind. The mutual aƈtions of bodies upon one another, eſpecially when their gravity is taken into the queſtion, depends upon ſo many conſiderations, and the caſes which may be put are capable of ſuch an infinite variation, that it is impoſſible almoſt to draw a general inference of this nature. Even when experiments are produced, which ſeem to prove the point, one is apt to ſuſpeƈt the univerſality of the concluſion, and to imagine that it may poſſibly be owing to ſome particular circumſtance which we have not attended to, or been able to diſtinguiſh from others not ſo eſſential. In the example we are conſidering it is clearly proved from experience, that $p \times V^2$ is equal to $pv^2 + \frac{Ev^2}{a^2}$; but whether that be true in every other caſe that may be conceived, can never be determined from ſuch an experiment; nor is it poſſible to make any diſtinƈtions about it, until we have demonſtrated its connexion with ſome other principle, which is more ſimple and leſs conteſted.

Retaining the ſame ſymbols, let F repreſent the force of gravity, and f the force which accelerates the body Q in its motion. From what has been already ſhewn it appears, that $F : f :: ap + bP : ap$ and $F - f : f :: bP : ap :: \frac{abPv}{a^2} : pv$; and becauſe pv is the motion generated in Q by the force f, $\frac{abPv}{a^2}$ will be the motion loſt in the ſame body Q by the diminution of its gravity. Let A be any priſmatic particle of the body, and AD its diſtance from the axis; the velocity of this particle will be $\frac{v \times AD}{a}$; its motion $\frac{A \times AD \times v}{a}$, and, by the nature of the lever, the motion which Q muſt loſe to generate ſuch an effect in A muſt be $\frac{A \times AD^2 \times v}{a^2}$. The quantity $\frac{vabP}{a^2}$ repreſents the ſum of all the quantities $\frac{A \times AD^2 v}{a^2}$; and therefore the motion, which Q has loſt by its action on the body, is preciſely equal to the motion gained by the different parts of that body after a proper allowance is made for the lengths of the levers, AD, &c.

Thus it appears, that there is no neceſſity in accounting for the time of Q's deſcent and the velocity it acquires, of having recourſe to the *conſervatio vis vivæ*, or any ſuch perplexed hypotheſis. By purſuing the analytic method far enough, we have been led directly to that fundamental law of motion, that action is equal to re-action, and in the contrary direction.

A diſtinction, however, is always to be made between the actions of bodies when at liberty, and when they revolve about a center or axis. In the firſt caſe the motion loſt is always equal to the motion communicated in an oppoſite direction: in the ſecond the motion loſt is to be increaſed or diminiſhed in the ratio of the levers before it will be equal to the motion communicated. The properties of the lever are well underſtood and eaſily applied, and becauſe their evidence depends upon experience, and is as firmly eſtabliſhed as the third law of motion itſelf, it is always beſt to make uſe of thoſe two univerſal principles, inſtead of others which are more liable to deceive us(b).

In all caſes concerning the motion of a ſingle body, or ſyſtem of bodies, where there is any rotatory motion, the conſideration of the lever becomes requiſite, and that, with a juſt application of the laws of motion, is ſufficient for the reſolution of the moſt arduous problems. It is now pretty well agreed upon, that the neglect of this circumſtance is one cauſe of that material error, which Sir ISAAC NEWTON himſelf is ſuppoſed to have fallen into in the thirty-ninth propoſition of the third book of his Principia.

(b) It is acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propoſe ſuch examples with a view rather of illuſtrating the methods of calculation by algebra and fluxions, than of making any uſeful improvements in practice. They ſuppoſe the particles of the fluid to move in ſtraight lines, and to ſtrike the machine with a certain velocity; and after that, to have no more effect. As ſuch ſuppoſitions are evidently inconſiſtent with the known properties of a fluid, we are not at a loſs to account for a difference between experiment and theory; and therefore it ſhould ſeem unreaſonable to aſſert, that certain authors of reputation have neglected the collateral circumſtances of time, ſpace, or velocity, in the reſolution of theſe problems, unleſs we were able to point out ſuch omiſſions.

I had ſeveral reaſons for inſiſting ſo particularly on the demonſtration of this third caſe. It is in itſelf one of the moſt neat and elegant problems we have; and, what is of more conſequence, it admits of an experimental proof and illuſtration. It is obvious, that the motion of the body AMB may be made ſo ſlow, that the time of Q's deſcent through any aſſignable ſpace may be meaſured to the greateſt exactneſs. The velocity of Q may alſo be inferred with the ſame eaſe by obſerving the velocity of any particular point in the body to which the velocity of Q always bears an invariable ratio. Such experiments, it muſt be owned, ſeem very unfit for the firſt diſcovery of the laws of nature; though, as I have ſhewn, it is not impoſſible to collect them that way; but after they are diſcovered, the application of them to the ſolution of ſuch intricate problems is both entertaining and inſtructive, and then the agreement of the experiments themſelves with the theory becomes a ſolid argument for the certainty of our principles.

We have ſhewn, that in this caſe at leaſt BERNOULLI'S hypotheſis is founded upon, and coincides with, the commonly received doctrine of motion, and therefore we can hardly entertain a doubt of the ſucceſs of the experiment, ſuppoſing it had never been tried. The author himſelf, in the paſſage above quoted, tells us, that he found it ſo; but we need not reſt upon his authority: a ſimilar experiment has been lately made by Mr. SMEATON, and is deſcribed at length in the Philoſophical Tranſactions, vol. LXVI.

It does not appear, that D. BERNOULLI attempted to meaſure any thing but the time of Q's deſcent through any particular ſpace: Mr. SMEATON has given us both the times of Q's deſcent, and the proportions of the velocities acquired, in a variety of caſes. By moving the weights he makes uſe of nearer to, or farther from, the center D, he alters the lengths of the levers at which the particles act, without increaſe or diminution of their number: he does the ſame with the circle or axis NMP, and conſequently the lever MD; and in every caſe, from the known character of that ingenious gentleman, we may preſume that his numbers are ſafely to be relied upon.

His concluſions may receive ſome illuſtration from the preceding theory.

From the proportion $F : f :: ap + bP : ap$, it appears, that the force which accelerates the motion of Q, or in Mr. SMEATON'S figure, the weight in the ſcale is to the natural force of gravity in a conſtant and invariable proportion as long as the quantities a, b, P, and p, remain the ſame; and therefore let Q deſcend ever ſo ſlowly, its motion will be uniformly accelerated throughout, and the ſpaces through which it deſcends will be as the ſquares of the velocities acquired, and the times will be as the velocities themſelves; and this is agreeable to what Mr. SMEATON found them in his ſecond and third, fifth and ſixth, eighth and ninth experiments.

The general expreſſion for the force which accelerates the weight in the ſcale is $\frac{ap+bP}{F \times ap}$, and will be different according as the quantities a, p, or b, are altered; but is always eaſy to be determined as ſoon as thoſe quantities are known. But it is impoſſible to determine the magnitude of the quantity b in the different caſes, unleſs we have given the preciſe dimenſions of the whole machine, and the ſpecific gravity of the wood made uſe of; and therefore I confeſs myſelf to have been puzzled in endeavouring to reconcile the firſt and ſecond and other experiments with the theory: for though I could not doubt a moment, that the general expreſſion for the force was rightly aſſigned, and would always be found conſonant to experience, yet I was extremely ſurprized to find, that when the quantity *a* in the ſecond experiment was made exactly one-half of what it was in the firſt, the time of deſcending through the ſame ſpace came out nearly double of what it was before, and the velocity the ſame. Now this I knew could never happen unleſs the force in the firſt caſe was to the force in the ſecond as 4 to 1; for when the ſpaces deſcribed are the ſame, the accelerating forces are always as the ſquares of the velocities, or inverſely, as the ſquares of the times. This conſideration led me to inquire farther into the ratio of thoſe forces in the caſe deſcribed, in order to diſcover, if poſſible, whether they came any thing near that ratio, which of neceſſity they ought to do.

I conſidered, that the weight of the axis and arms of the machine was inconſiderable, compared with the weight of the two cylinders of lead, and alſo that the quantity *a* bore a very ſmall proportion to the length of the cylindrical arms of fir. And ſince the accelerating force is always as $\frac{ap}{ap+b\text{P}}$, or as $\frac{a^2p}{a^2p+ab\text{P}}$, and the quantity $ab\text{P}$ or E expreſſes the ſum of all the particles multiplied by the ſquares of their diſtances from the axis of motion, it is plain that E muſt far exceed a^2p; and, laſtly, ſince the quantity E is the ſame both in the firſt and ſecond experiment, it follows, that the forces are very nearly to one another as a^2p to $\frac{a^2p}{4}$, or as 4 to 1: and in the ſame way the other experiments are ſhewn to be conſiſtent with the theory.

I choſe to premiſe a ſhort account of the opinions which the philoſophers before GALILEO entertained concerning the motions of bodies; becauſe their miſtaken ideas of the effects of gravity are analogous to ſome opinions of a later date, which indeed ſuggeſted the neceſſity of reſuming theſe inquiries.

And as nothing in controverſial matters ſo completely ſatisfies the mind as an exact knowledge of that particular which produces the diſpute; I have ſhewn, that the terms made uſe of to expreſs the third law of motion were taken in two very different ſenſes: that Sir ISAAC NEWTON's explication of them is at beſt ambiguous, and MACLAURIN's abſolutely falſe.

1ſt. In the demonſtration of the firſt caſe we ſee that the aſſertion of LEIBNITZ is true in one particular inſtance. When two elaſtic balls move in the ſame ſtraight line, the ſum of their forces is not altered by colliſion; and it is more than probable, that this ſingle circumſtance was the cauſe of affixing new ideas to the terms action and re-action. For, 2d. In the ſecond caſe, the ſame principle is taken for granted by J. BERNOULLI. We have examined into the conſequences of this author's ſolution, and ſhewn that his hypotheſis will prove all bodies to be perfectly elaſtic. As the ſteps by which he deceived himſelf are here expoſed, whoever carefully attends to theſe two examples cannot eaſily miſtake in any caſe that may occur. It is plain, that if any one contends for the equality of action and re-action, and explains thoſe terms by the changes produced in the abſolute forces of the bodies, the diſpute is not merely verbal.

3d. When a concluſion, agreeable to experience, is deduced from any hypotheſis, it does not therefore neceſſarily follow, that the hypotheſis is univerſally true, not even ſuppoſing the converſe of the propoſition to hold. In this third caſe it is ſhewn, what kind of anſwer we are to give ſuch reaſoning. The *conſervatio virium vivarum* is never to be admitted, unleſs its connexion with ſimple facts, which are inconteſtable, be firſt made out. The ſolution of this problem depends on this, that the motion loſt is equal to the motion communicated in a contrary direction after the property of the lever is taken into the account; and therefore the nice agreement of Mr. SMEATON's experiments with the theory cannot fail to add freſh evidence to theſe eſtabliſhed laws of nature.

I ſhall conclude theſe remarks with obſerving that ſince it is perhaps impoſſible to give one general anſwer to all the arguments which are brought in favour of the new doctrine of forces, it ſeemed very deſireable that we ſhould have a general rule to direct us in judging of the caſes that occur in practice. It is of more conſequence to the improvement of ſcience and the good of the public, to point out the ſource of miſtakes, and the wiſeſt means of avoiding them for the future, than merely to confute and ſilence our adverſaries. Some writers have conſidered this queſtion as entirely verbal, and have affected to treat the advocates on both ſides with the greateſt contempt. Such perſons ſave themſelves a great deal of trouble, and have the credit of ſeeing farther into the controverſy than others; but after all, I am afraid the practical mechanic will receive little information or ſecurity from ſuch ſpeculations. Propriety of expreſſion in theſe matters is not all we want. When a plan is propoſed for execution, and a certain effect predicted, the grand object is, how to form a ſure judgement beforehand of the event, in order to prevent unneceſſary expences; and I ſhall think my time well employed, if theſe conſiderations appear to have the leaſt tendency to promote ſo uſeful an end, in the opinion of that Society to whoſe learned and zealous endeavours we owe the very firſt important diſcoveries in the year 1668, concerning the colliſions of bodies.